

In situ Cosmogenic ^{10}Be and ^{26}Al Reveal a Complex Exposure and Erosion History of the Landscape Once Covered by the Quebec-Labrador Ice Dome

Cavnar et al. have provided a compelling study using paired in situ cosmogenic nuclide (^{10}Be and ^{26}Al) measurements to deglacial sediments, modern river sands, and bedrock within the region formerly occupied by the Quebec–Labrador Sector of the LIS. This investigation includes an extensive dataset of previously published nuclide concentrations from 238 bedrock and boulder samples from across the Quebec-Labrador Sector. Their objective is to constrain the erosional efficacy of the LIS in this sector and to evaluate the extent of nuclide inheritance—cosmogenic concentrations acquired during pre-LGM exposure and preserved through subsequent glaciations and its implications for future cosmogenic nuclide age determination studies.

From their results, deglacial sediments (100%), boulders (34%), and bedrock surfaces (57%), all contain inherited nuclides. Additionally, the similarity of nuclide concentrations between modern river sand and deglacial sediments demonstrates that paraglacial fluvial systems recycle glacial deposits with minimal contribution from fresh soil or recently eroded bedrock. A single bedrock depth profile possibly suggests the presence of inherited nuclides.

Major recommendations: Given the challenging nature of this investigation and the vast remote region it encompasses; some assumptions and generalizations were understandably necessary. However, I think at times the findings are overstated given these limitations. One of the main generalizations is that the Quebec-Labrador Sector is represented as a single dome with static radial flow throughout the entire glacial cycle and placing its centre near Lab City. I acknowledge that some generalization has to be made for the study, but acknowledging this in the manuscript is required. The Quebec-Labrador sector was a vast ice mass with multiple ice saddles and divides with ice flowing in multiple directions which makes determining the "centre" of it difficult. The best case for the closest centre to this study I think has been made by Dyke and Veillette who place it relatively near the community of Schefferville or further north. The current centre that is used is more a reflection of the centre of ice-margin retreat, which could be argued to control meltwater flow and tie into the glaciofluvial delta samples, but equating that centre with erosional intensity and the ice sheet's ability to remove inheritance is misleading. This needs to be clearly clarified in the manuscript. Additionally, I believe that some of the samples classified as boulder and bedrock samples should not be used for assessing the Quebec-Labrador sector, as they may have been sourced/eroded from other sectors.

With that being said, the overall findings based on the data presented are valid and provide an important caveat that all researchers who collect cosmogenic nuclide deglacial data should be cognisant of when collecting such samples. I am impressed with the degree to which the manuscript has been improved and the effort that the lead author put forth to facilitate these changes.

I have provided specific comments by line below. Please don't hesitate to reach out if you have additional clarification questions.

Jessey Rice

Specific recommendations:

Line 19: I think you should refer to this as the Quebec-Labrador sector. Dome implies a single centre with radial flow, which vastly oversimplifies the actual flow morphology of the ice sheet. It was composed of multiple saddles and divides, from which ice propagated. Setting aside that these saddles and divides migrated through time (which should be discussed briefly in the manuscript).

Line 20 -21: Stating deglacial eskers is a bit redundant, just eskers would work. Being consistent with the number of samples would also make it easier to read. i.e., we sampled esker and delta sands (n=10), modern streams (n=11), and bedrock (n=1).

Line 28: I am not sure 65/192 (34%) would be considered common, this should be rephrased to not oversell the findings.

Line 38: Again, stating many is a bit misleading when you group in the boulders with the bedrock, together they are less than 50%.

Line 44: e.g., not e.g,

Line 48: e.g., not e.g,

Line 59: I think saying the deglacial history has been established is not quite accurate, we have made incredible gains but there are a lot of questions remaining regarding the deglacial pattern and chronology of the Quebec-Labrador sector.

Line 61: A bit of an awkward sentence, possible revisit.

Line 83: White circles, not open circles.

Line 84: Should cite the Margold paper from which this data was taken.

Line 100: Figure E. from this image it appears that the bedrock profile was taken from a small bedrock scarp. It is difficult to say for certain from the image, but if so, would this not expose the lowest sample in the profile to quite a bit of cosmic bombardment since its exposure? Or is this a drilled exposure along the highway? Please provide a bit more context, the findings from this profile are significant to the overall conclusions, so more discussion on it is warranted in my opinion.

Line 177: I know it is mentioned later in the manuscript, but I think it is better to state here why you did not use the 2023 reconstruction, this sets the foundation better for later discussions.

Line 180: Missing the ice sheet (The margin of the then retreated).

Line 196: I think it's important to reference the Dyke reconstruction as well here. There were few or no changes in many places of the eastern sector during that update.

Line 203-204: Analytical uncertainty could also be the reason no? They are commonly in the multiple century scale.

Line 266: Nelson et al. is missing a year

Table 1: I think glaciofluvial delta is more accurate than glacial delta.

Line 299: ICP-OES: inductively coupled plasma optical emission spectrometry

Line 315: 2850×10^{15} seems like an odd way to report this, given how all of the other ^{10}Be abundances are reported, should be 2.850×10^{12}

Line 352: My understanding of a Tukey test is that it assumes the data is normally distributed. But you state earlier in the sentence that the data is non-normal in its distribution. I am wondering if this is the best test for this dataset, or the authors have transformed the data prior to testing it, which should be stated. This should be addressed either way.

Section 4.5: Were any corrections for shielding incorporated? If no, why not? If there was not shielding it should be stated. Although based on the photos there should be some topographic shielding. I am assuming there will be significant snow cover as well based on 158 mm of snow water equivalent stated in section 2.

Line 371: Why were older constants used for the deglacial sediments? Why were the 2024 constants not used?

Line 374: I assume you used a density of 2.65 for the bedrock samples? Should state to be clear

Line 374: Did the deglacial and modern sands have the same density?

Line 404: Missing a space between 21of

Line 414: Given that the two labs had different uncertainties from their blanks, it would be easier to assess the data if the labs were indicated in the table.

Line 446: Again, saying this is common when it is in less than half of the combined samples (bedrock and boulders) is misleading.

Line 449: This is logical given the erratics are being transported subglacially and are susceptible to erosion from multiple sides where the bedrock is just from the surface.

Line 459: The reference to the centre of the QLID to being near Lab city is a bit misleading as this was the location where ice retreated toward, but was not the “centre of the dome”, we have tried to move away from the use of the term dome and centres as these ice sheets were not as simple as a single source of ice flow radiating away, but in fact composed of multiple ice divides which migrated throughout glaciation. However, for this evaluation of the data I realize it is unrealistic to have multiple migrating points of an ice centre. Therefore, I think the general middle, or confluence of many of these ice divides was somewhere between Schefferville and Kuujuaq and should be used instead (see: Andrews & Miller (1979) and Dyke & Prest (1987). Additionally, based on dispersal patterns and ice-flow erosional indicators, the closest divide is further north (Klassen and Thompson, 1993). I do not think this will affect the conclusions you make, but it is a more accurate placement of the centre.

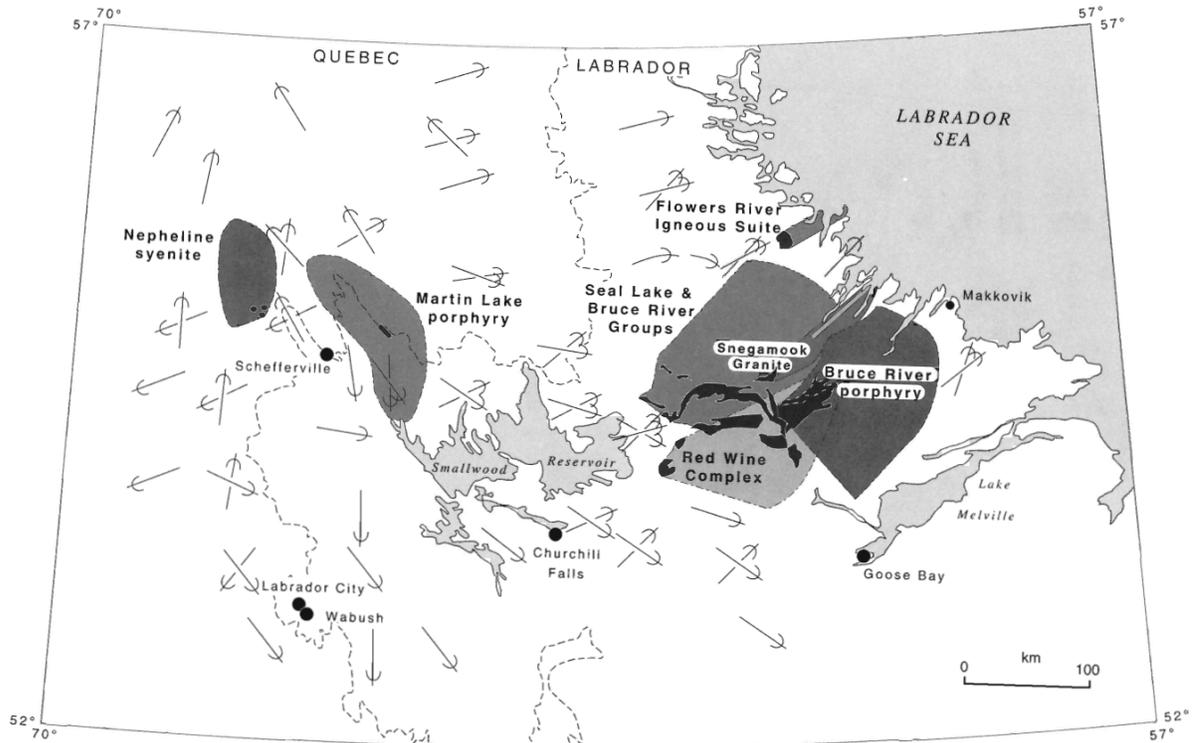


Figure 20. Indicator erratics derived from six distinct bedrock sources define glacial dispersal trains in central Labrador. The trains vary from narrow ribbons of outcrop width near the ice sheet margins, where ice flow has been relatively simple, to broad fans and patches near the locations of former ice divides in western Labrador, where ice flow has been most complex. Data are derived from field mapping of clasts (see Fig. 21 a- for detail of dispersal trains).

And from Veillette et al. (1999)

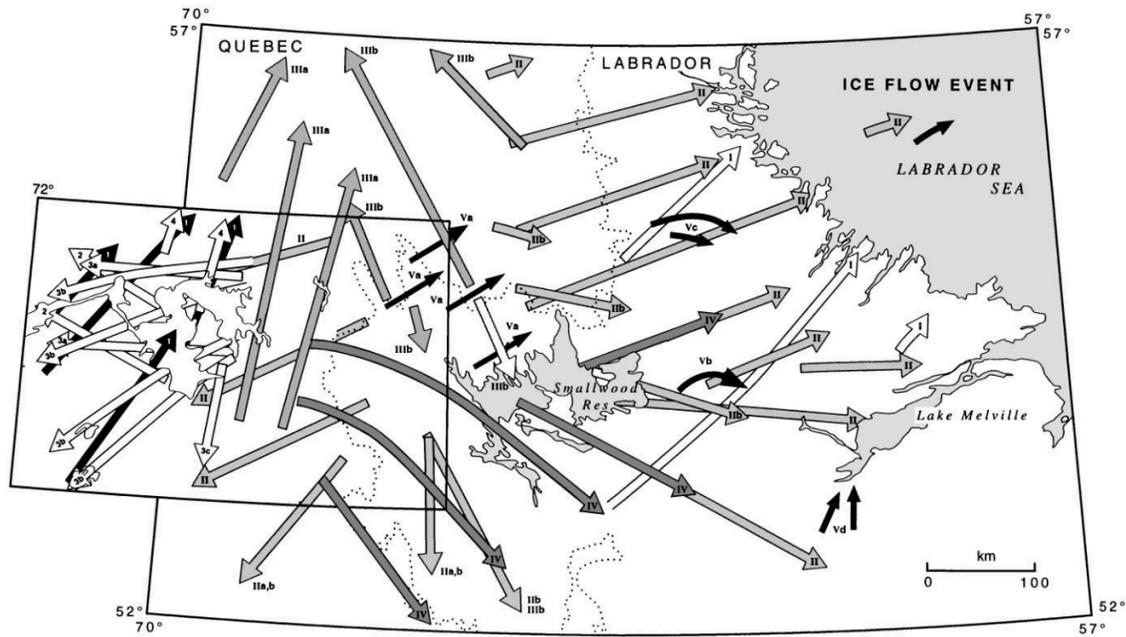


Fig. 13. Ice flow chronology of Caniapiscau area (Fig. 12) correlated with that developed by Klassen and Thompson (1993) for a larger area in Labrador.

Line 479: If the uncertainties are 1σ , why are some not equal? Just provide some clarification in the text on this.

Line 489: I think there should be an addition of clarification that not as many boulders. As it reads, you are suggesting that many of the boulders contain inheritance when in fact it is only 34%.

Line 497: The line Bias comso exposure ages high reads a bit awkward. Please adjust.

537: Need a year on the Dalton reference.

555: The referenced articles are from the Torngat Mountains- well outside the study area (over 500 km away), with no sediments or boulders being transported from that area to the study area (all toward the Labrador coast). These studies also specifically targeted areas of cold-based erosion where inheritance would be high.

560: Need a year on the Melanson et al reference.

Figure 8: I have some issues with some of the samples included in this dataset. Please see supp. Data section at the end.

Line 622: grey for consistency.

Line 630: Provenance studies of IRD could support Hudson Strait source and persistence of QLS, although their samples were a bit north: <https://doi.org/10.1016/j.quascirev.2009.08.008>

643-44: Difficult to average such a large field area down to a single age, would it not be better to do this in groups? At least two different groups given your Figure 1 and two sets of samples spread over 3.5 ka. More groups if feasible.

647: Where was this taken from? Knowing that modern sediment contains on average 33.1×10^3 ^{10}Be atoms g^{-1} ,

649: Would a modern river erode enough bedrock to account for a component of modern sediment deposition? I think the bedrock could be removed here.

Line 656: in a single bedrock profile.

Line 658: Again, the use of commonplace is misleading.

Line 661: Alternatively, the regolith that was removed was not thick enough to absorb all of the cosmic bombardment. There is very little regolith left from interglacial periods and most are in the far north of the QL sector, so stating it was not completely removed, while accurate, is a very minor portion of the landscape.

667-668: I think this is an important finding and justifies assuming zero postglacial erosion on the bedrock sample, which could be stated earlier than the conclusions.

Supplementary Data

There are no references for any of the references given in the text which made evaluating the data difficult but some notes:

Pico et al. (2021).

This paper was rejected. As per the journal's guidelines, it should not be used.



18 Oct 2021

Status: this discussion paper is a preprint. It has been under review for the journal *Climate of the Past* (CP). The manuscript was not accepted for further review after discussion.

Was there a glacial outburst flood in the Torngat Mountains during Marine Isotope Stage 3?

Tamara Pico [✉](#), Jane Willenbring, April S. Dalton, and Sidney Hemming

Gosse et al. 2006:

This reference is for the island of Newfoundland- it has its separate independent ice sector during the last glaciation which was unique in its own sense. I am not sure this data should be used for this context. It certainly was not part of the Quebec-Labrador sector.

Godbout et al. 2017:

These samples were collected from a location with a more complex depositional history than just being sourced from the Quebec Labrador sector- could equally likely to have been sourced from the Keewatin sector of the LIS. From their publication:

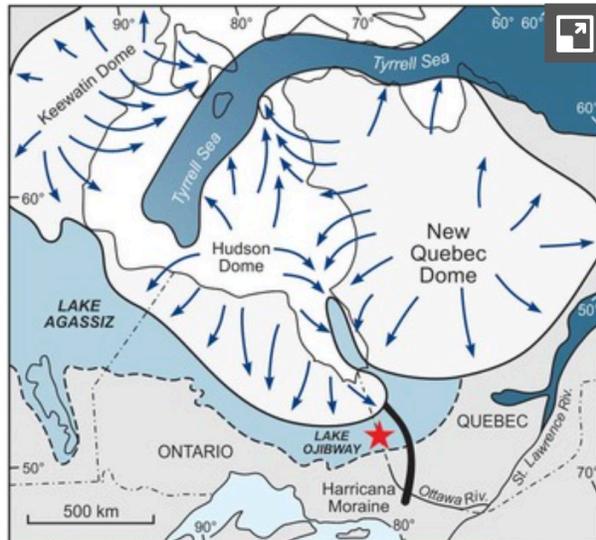


Figure 1 (A) Schematic extent of Lake Agassiz and Lake Ojibway in the context of the Laurentide Ice Sheet at ~8.5 cal yr BP (Dyke et al., 2003; Dyke, 2004); location of the study area (red star). (B) Study area and main physiographic features of the Ojibway basin in Ontario and Quebec. Note that the red box covers the area shown in Fig. 3. Triangles show the location of the outlet system related to the main stages of Lake Ojibway (orange, Angliers; blue, early Kinojévis; white, late Kinojévis; after Vincent and Hardy, 1979). (For interpretations of the references to color in this figure legend, the reader is referred to the web version of this article.)

These are not viable samples for assessing inheritance of the Quebec Labrador sector.

Clark et al. 2003 and Marquette et al. 2004:

These samples were collected from Alpine terrains that, especially during the moraine formations, were likely more topographically constrained to smaller geographical regions and the boulders sampled were unlikely to be sourced from the interior of the Q-L sector. At least equating these high elevation locations to the rest of the Q-L sector is misleading, they should be reported with caution if used at all.